Franklin's Scientist Enemies: Real or Imagined

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The title of my paper indicates that my subject is not "Franklin's *scientific* enemies," since I am not at all sure that I know what qualifications make an enemy "scientific" rather than "un-scientific" or "non-scientific." The topic of "scientist enemies," however, suggests the possibility of some kind of enmity among scientists at home and abroad. That is, there were individuals who were both scientists and enemies, even though their "science" was not in the status of "enemy" with Franklin's science. As we have become aware since the publication in 1962 of Thomas S. Kuhn's study, *The Structure of Scientific Revolutions,* there are no contests between scientific theories or scientific ideas, but rather between scientists who advocate scientific theories or believe in scientific concepts—that is, between scientists. Thus, like all creative scientists, Franklin had many fellow scientists who disagreed with his theory, but in the restricted and exact sense of the word, it is difficult to find fellow scientists who would qualify an "enemies."

In the realms of science there are always opponents or contestants, both proponents of rival theories or concepts and rivals for the honor or priority of discovery or invention of new concepts, principles, theories, or their applications. Only rarely, however, does such a contest depart from intellectual or "scientific" competition and become so acerbic that the rival figures become enemies in any usually accepted meaning of that word. In such cases, the issues cease to be "scientific" and degenerate into personal attacks, accompanied by the impugning of a rival's probity or general character.

I know of only one clear case of such enmity in Franklin's day, a bitter quarrel that reached its peak during the years of Franklin's youth, the enmity produced by the intense controversy over the invention of the calculus. It is well known today that both Isaac Newton and Gottfried von Leibniz simultaneously and independently invented the calculus, without doubt the most important innovation in the development of modern mathematics. Newton was the first to develop the calculus, but Leibniz was the first to publish an account of the new mathematics.

When Leibniz published his version of the calculus, Newton and his cohorts simply could not imagine that Leibniz—or anyone else—could independently have conceived the concepts of the calculus, or could have had the same ideas as Newton. Therefore, they concluded that Leibniz must have had access to Newton's unpublished manuscripts and have plagiarized them. Newton's mathematical results had been circulating in England, but the papers to which Leibniz had access dealt with Newton's work on infinite series and not the mathematics of the calculus. Newton did send Leibniz two statements about his work in the calculus, but these were sent in cipher or code that would have been totally indecipherable, even to a genius of Leibniz's stature.

After Leibniz had published his version of the calculus, when Newton's cohorts publicly declared Leibniz to have been a plagiarist, Leibniz, of course, flatly denied the charge. He demanded that Newton, the President of the Royal Society, proclaim his innocence. Newton's response was to appoint a so-called international commission of the Royal Society to investigate the matter.

We now know that all the members of the commission were ardent supporters of Newton, that the commission was "stacked" so as to yield a verdict against Leibniz. And indeed their published report flatly declared Leibniz to have been a plagiarist, a thief who had stolen the calculus from Newton, the "first inventor."

Enmity looms large in Newton's subsequent behavior. He secretly wrote the commission's report, as we now know on discovering the successive drafts in Newton's own handwriting among his personal papers. His manuscripts also show that, once the report of the commission had been published, Newton then wrote a long review of the report, summarizing its findings and praising the work of the commission. This report appeared anonymously in the official journal of the Royal Society, the *Philosophical Transactions.* Newton, as president of the Royal Society, later authorized a reprint of this report to which his review (still anonymously authored) was affixed as an introduction; and to make sure that readers got the message about the plagiarism of Leibniz, Newton wrote a short preface which was also published anonymously. His hatred for Leibniz extended to the grave. Some years after Leibniz died, Newton still expressed his vengeful hate. He was reported to have once "pleasantly" told Samuel Clark that "he had broke Leibniz's heart with his reply to him."

Newton's example is extreme, but—being the kind of genius he was—everything about Newton was extreme. He was a quarrelsome man. His fierce controversies with Robert Hooke (the curator of experiments at the Royal Society and an inventor of the pendulum clock) and John Flamsteed (first Astronomer Royal) are part of the notorious underside of the history of science. The seventeenth century, however, was an age of quarrels. So many new scientific discoveries were being made in rapid and overlapping succession that priority rights and plagiarism charges flew left and right. Leibniz, who was accused of plagiarism by Newton in the invention of the calculus, in turn accused Descartes of plagiarizing the concept of celestial vortices from Kepler. Other scientists accused Descartes of plagiarizing the law of refraction from Willebord Snel. Today we are aware, thanks to the perceptions of Robert Merton, that such incidents come from a common feature of the advance of science: that discoveries tend so frequently to be multiple and independently made that this may be considered a normal feature of science. We may understand, therefore, why there have been so many priority contests in science. But I am not at all certain that such priority contests necessarily produce a condition of scientific enmity—at least in the sense in which the term "enmity" is usually understood.

Franklin himself was quite bitter on this subject of priority and plagiarism. It was a primary topic in a celebrated letter to Dr. John Lining of Charleston in March of 1755, in which Franklin discussed scientific creativity and in which he copied an extract from his diary (now lost), recounting the steps that had led him to the idea that the lightning discharge is an electrical phenomenon. "The treatment your friend has met with is so common," Franklin wrote, "that no man who knows what the world is, and ever has been, should expect to escape it." The friend had evidently had the experience of many inventors or discoverers. "The smaller your invention is," Franklin continued, "the more mortification you receive in having the credit of it disputed with you by a rival, whom jealousy and envy of others are ready to support against you, at least so far as to make the point doubtful." If you choose "not to dispute the point, and demonstrate your right," he explained, "you not only lose the credit of being in that instance *ingenious*, but you suffer the disgrace of not being *ingenuous*; not only of being a plagiary but of being a plagiary for trifles."

Almost every stage of Franklin's scientific career was marked by some kind of rivalry and by claims of invention made by or for rival scientists. A brief review of some of these may be useful for a better understanding of the problem at hand. Franklin gained world-wide fame as an experimenter and a theorist. His experiments not only revealed a host of new phenomena then unknown to the "electricians" of Europe; they were phenomena of a special kind to the extent that they had a significant bearing on the development of fundamental theory. In his first communications on the subject, in the form of letters addressed to his English correspondents, primarily Peter Collinson (a British Quaker merchant who was very active in promoting the intellectual interests of Philadelphians), Franklin merely described the many new phenomena that he and his co-experimenters in Philadelphia had been finding, together with the explanations that had been devised and that were being welded together to form the Franklinian theory of electrical action. He had no idea that they would be collected by Collinson and published as a little booklet, entitled *Experiments and Observations on Electricity*.

Once these letters had been printed, however, Franklin was acutely aware that—as letters—they had not specified in detail who had made certain particular discoveries, which should have been done if they were to be considered as public epistolary discourses. Accordingly, in the revised editions he entered notes which he had carefully entered into his own copy of record, indicating who had actually been the first to make certain observations. For example, he added a note reading:

> This Power of points to throw off the electrical fire, was first communicated to me by my ingenious friend Mr. Thomas Hopkinson, since deceased, whose virtue and integrity, in every station of life, public and private, will ever make his Memory dear to those who knew him, and knew how to value him.

Another note read:

> This was Mr. Hopkinson's Experiment, made with an expectation of drawing a more sharp and powerful spark from the point, as from a kind of focus, and he was surprised to find little or none.

Yet another such note read:

> These experiments with the wheels, were made and communicated to me by my worthy and ingenious friend Mr. Philip Syng.

In describing the invention of the "magical picture," he made two such references in notes. Let me remind you what the "magical picture" is by quoting Franklin's description:

> The magical picture is made thus. Having a large metzotinto with a frame and glass, suppose of the King (God preserve him) take out the print and cut a panel out of it near two inches distant from the frame all round.

The picture was to be put back into the frame in such a way that there was a backing of gold foil and some gilding on the front glass, with suitable electrical contacts so that it formed plate capacitor or condenser.

A capacitor or condenser is an electrical device essentially consisting of two parallel metal conductors separated by a non-conductor. One familiar form of capacitor consists of a glass plate with metal coating on each side. In Franklin's day, the most common form of capacitor was the "Leyden jar," a glass bottle (the non-conductor) filled with lead shot or water (the inner conductor) and with a coating of metal outside (the outer conductor). This device, invented around the middle of the eighteenth century, had the astonishing ability to store up and then discharge a prodigious amount of "electricity," thus enabling showman demonstrators to produce large-scale demonstrations for the delectation of their audiences. One of Franklin's most significant innovations in electrical science was to explain the action of the Leyden jar.

In the case of the "magical picture," just such a capacitor was formed and could be charged up to a considerable degree. A small "moveable gilt crown" was to be placed on the king's head. Then, the "magical picture" was ready for use, described by Franklin as follows:

> If now the picture be moderately electrified, and another person take hold of the frame with one hand, so that his fingers touch the inside gilding, and with the other hand endeavour to take off the crown, he will receive a terrible blow, and fail the attempt.
> If the picture were highly charged, the consequence might perhaps be as fatal as that of high treason. . . .

Franklin added, with characteristic dry humor, "If a ring of persons take the shock among them, the experiment is called *The Conspirators.*"

In the later printed editions, Franklin introduced two important notes concerning the magical picture. One was related to his invention of the plate capacitor or condenser, concerning which he wrote: "I have since heard that Mr. *Smeaton* was the first who made use of panes for that purpose." William Smeaton was a British inventor, noted for his improvements of pumps and steam engines. Franklin acknowledged that Smeaton had the priority in the invention of the plate capacitor, exemplified in the "magical picture" by two surfaces of gold leaf separated by the glass of the picture frame. Having acknowledged the independent discovery of the plate capacitor, Franklin turned to the "magical picture." He noted, simply and straightforwardly, that it has been "Contrived by Mr. *Kinnersley.*"

Later on, Franklin took particular note that it had been Kinnersley who had raised anew in an important way the problem that negatively charged bodies repel one another. Let me note, in this connection, that one of the primary deficiencies of the Franklinian theory of electrical action was that—while it could explain and predict the outcome of almost every kind of electrostatic manipulation and the induction of charges—it actually would fail in this one regard: it would not predict, nor could it explain, why two negatively charged bodies repel each other. And so, Kinnersley's experiments on the repulsion between two negatively charged bodies were important in establishing the fact of such repulsion and removing any possibility of doubt that Franklin's theory of electrical action had a deficiency that needed repair.

In order to appreciate the significance of this defect in Franklin's explanation of electrical phenomena, it is necessary to review some of the fundamentals of that theory. Franklin supposed that electric matter is made up of particles which repel one another and that the particles of ordinary matter and electric matter attract each other. A positively charged body, in Franklin's terms, is one that has gained excess electric matter, while a negatively charged body is one that has lost some of its normal electric matter. Clearly, a body that has lost some of its electric matter will attract electric matter (or a positively charged body, one with a super-abundance of electric matter) in an attempt to restore the balance. Thus a negatively charged body will attract and be attracted by a negatively charged body. Similarly, the excess electric matter in two positively charged bodies will cause a force of repulsion. But why should two negatively charged bodies repel each other? Franklin could never find an adequate reason.

Rather than conceal the problem, Franklin actually published (in the supplementary part of the later editions of his book on electricity) Kinnersley's own presentation of his experiments and his challenging point of view. Here is what Kinnersley had to say:

> The doctrine of repulsion, in electrised bodies, I begin to be somewhat doubtful of. I think all the phaenomena on which it is founded, may be well enough to account for without it.

Franklin's reply began:

> You know I have always looked upon and mentioned the equal repulsion in cases of positive and of negative electricity as phaenomenon difficult to be explained. I have sometimes, too, been inclined with you, to resolve all into attraction.

But, Franklin noted, he had found that there "are likewise appearances of repulsion in other parts of nature." He ended with a bit of self praise:

> You see I am willing to meet you half way, a complaisance I have not met with our brother *Nollet*, or any other hypothesis maker, and therefore I value myself a little upon it. Especially as they say I have some ability in defending even the wrong side of the question, when I see fit to take it in hand.

The Nollet in question was the Abbé Nollet, who—we shall see in a moment—was possibly the nearest Franklin came to having a scientist enemy.

Referring to another set of experiments by Kinnersley, Franklin wrote:

> your . . . . beautiful experiment on the wire, which you made Hot by the electric explosion, and in that state fired gunpowder with it, put it out of all question, that heat is produced by our artificial electricity, and that melting of metals in that way, is not what I formerly called cold fusion.

We cannot read this extract in the 1990s without being reminded that we too have, like Franklin, witnessed the demise of supposed experiments that had seemed to demonstrate the possibility of "cold fusion."

In one of Kinnersley's letters, he referred—in what Franklin called a "joking manner"—to "electrical orthodoxy." This phrase embodies a brilliant perception of the nature of the struggle of new ideas and new theories to gain a foothold against the currents of orthodox thought held by members of the scientific community. Franklin and his associates were rebels against the established or orthodox views. In experimental science, as Franklin wisely observed, "Opinions are continually varying, where we cannot have mathematical evidence of the nature of things; and they must vary." He added, with characteristic perception, "Nor is that variation without its use, since it occasions a more thorough discussion, whereby error is often dissipated, true knowledge is encreased, and its principles become better understood and more firmly established."

Let me now turn to a second aspect of possible enmity in the sciences. An aspect of science that is very apt to turn rivalry into bitter enmity is the matter of patronage or of control of funds and laboratories or of power to determine the direction of research. These are all areas where scientific action or behavior is not characterized by the ideals of an abstract search for truth. Within the organized structure or social system of science there occur the same kinds of power struggles and problems of control that characterize other forms of social interaction and organization. And this is all the more the case whenever the scientific area involved is apt to be fiercely competitive, or closely related to practical issues of economic importance, national defense, or public health.

We may see how this type of situation can produce a condition of enmity by briefly examining the case of Lysenko and Lysenkoism in the days of Stalinist tyranny in the Soviet Union. Lysenko was a non-academic unorthodox plant breeder who developed a theory of genetics that was officially declared to be a rival to what he and his followers called the "bourgeois" Mendelian genetics of what they considered to be "capitalist countries." This theory of Lysenko was pronounced by Stalin to be the only "true" theory and every geneticist in the Soviet Union had to abandon his or her scientific training and adopt the idea of Lysenko—or else! Lysenko's system of genetics was based, among other things, on a supposition of the possibility of the inheritance of acquired characteristics. Not only was the possibility of such inheritance denied by orthodox genetics, but there never has been an example of such inheritance found in nature or in the laboratory.

In the Soviet Union there was more at stake than just questions of "pure" science—alternative or rival theories of genetics and the possibility of the inheritance of acquired characters. Lysenko and his followers, with the personal intervention and support of Stalin, gained control of the vast network of agricultural experiment stations, with all their funds and personnel. Further, this group controlled the biological division of the Soviet Academy of Sciences and appointments to teaching posts in the universities. Even more important, in those days of the perpetual problem of the food supply in Russia, Lysenkoism by decree replaced Mendelian genetics as the new orthodoxy in all work on improving the food crops in the Soviet Union. There was at first a brief power struggle and some concerted resistance to Lysenkoism, but the resisters were speedily overcome by the force of Stalin's overt support of Lysenko. Those who could not abandon their true science were cast in prison and ended up in the Gulag. They saw their life-work and career being destroyed. Certainly, in this extreme case, orthodox geneticists must have seen Lysenko as both a political and a scientific enemy and, contrariwise, he must have seen them in the same role.

Benjamin Franklin was once unwittingly thrust into a somewhat similar situation, although one in which the stakes were many orders of magnitude lower. The occasion was the introduction of his ideas in France, following the translation of his book on electricity into French. Little did Franklin know that the promotion of his book by the leading French naturalist, the Count de Buffon, was part of a patronage feud between Buffon and the Abbé Nollet.

At the time Nollet was not only the leading figure in electrical science in France and perhaps the Continent, but was also considered to be the "dean" of French experimental scientists. His books, containing the results of his electrical research and his own theory of electrical action, were issued in many editions and in a number of translations. He was also the author of a work on the methods of experimental science which was considered to be so fundamental that it was even translated into English at a time when it might be thought that Newton's *Opticks* would have been considered a sufficient guide. In addition Nollet wrote a multi-volumed compendium of physical science that was often reprinted and was considered a standard reference on many different aspects of physical science.

In the 1750s, when Franklin's book was translated into French, Nollet held two important social or political positions. He was professor of physics at the great military school at Méziéres and he had he honor of demonstrating principles of science before members of the Court and the King of France, being especially popular with the ladies.

Nollet's rival, Buffon, was the superintendent of the Royal Garden and one of the world's leading naturalists. He was an important figure in the Court. He would personally conduct the King and his entourage through the Royal Garden and he would regularly present to the King the latest volumes of his splendidly printed and elegantly illustrated *Histoire Naturelle*, sumptuously produced by the Royal Printing House.

Buffon's intense rivalry with Nollet—shall we call it enmity?—extended from questions of patronage to questions of science. Although Buffon is known today primarily as a naturalist, he was an able mathematician who translated Newton's mathematical writings into French and he made some significant contributions to probability theory.

At the time that Franklin's book was first published, Buffon had long been interested in electricity. In fact, just before he read Franklin's book he wrote an account of the subject which must have seemed like a direct "slap in the face" to Nollet. Rejecting as inadequate all pre-Franklin attempts to explain electrical phenomena, including Nollet's theory, Buffon wrote that the subject of electricity was "not yet sufficiently ripe for the establishment of a course of laws or indeed or any certain law, fixed and determinate in all its circumstances."

Quite obviously, Buffon's sponsorship of a translation of Franklin's book was part of his program against Nollet, presenting to the French scientific public a rival theory which actually set forth laws that were indeed "certain" and "determinate" in all circumstances. That the book was intended as an affront to Nollet must have seemed obvious from the fact that Nollet was not even mentioned in the historical introduction by the translator, who said he was tracing the history of the subject "from its origin up to the discoveries of Monsieur Franklin."

The conflict into which Franklin had unwittingly been thrust became more intense when the Franklin experiments, described by the book, were performed at the Chateau de St.-Germain in the presence of King Louis XV, while Nollet was off teaching the principles of physics to military cadets at Méziéres. This time it was a favorite of Buffon named Monsieur Delor (or De Lor) who was performing experiments before the King, whereas usually it had been Abbé Nollet who had entertained the King and Court with experimental electrical entertainment.

The King, having witnessed the experiments and having heard of Franklin's ideas, then asked Buffon about what the French called *the* Philadelphia experiment. This was the experiment that Franklin devised to test his hypothesis that the lightning discharge is an electrical phenomenon. In this experiment, an experimenter was to be placed in a kind of sentry-box, protecting him from the rain during a storm. From this box, a long pointed rod would protrude. Protected by sufficient grounding, the experimenter would see whether or not this apparatus would "draw electrical fire" from passing clouds and thus prove that clouds are electrified. One of the principal discoveries of Franklin and his fellow-experimenters in Philadelphia had been this "power" of points. In the laboratory, it had been found that when a pointed and grounded conductor, such as a sharp wire connected to a pipe going into the ground, was placed near—but not in actual contact with—a charged body, it would "draw off" the charge and so produce a complete discharge.

Franklin's proposed experiment of the sentry-box was based on his desire to test his conclusion that clouds are electrified. This experiment was not designed to attract lightning, but rather to see whether a pointed grounded conductor could draw off charge from passing clouds, thus showing that clouds are indeed electrified. It would then follow that, because clouds are electrified, they can produce a large-scale electric discharge or lightning.

The goal of the experiment, then, was to find out whether Franklin was correct in concluding that clouds are electrified and that, therefore, the lightning discharge is an electrical phenomenon. Franklin described the proposed experiment in his book, but he had been waiting for the completion of the spire on Christ's Church in Philadelphia so that he could have a kind of sentry-box as high as possible. Let us note, in passing, that the proposed experiment of the sentry-box was the primary experiment on the nature of lightning, rather than the experiment of the lightning kite, which he conceived later on, only after the book had been published.

The King's question induced several French scientists, including Dalibard (who had translated Franklin's book into French) and Delor (who had performed the Franklin experiments before the King) to address themselves to the Philadelphia experiment. They set up the apparatus and met with immediate success. They reported to the French Academy of Sciences on the following 10th of May and 18th of May respectively (1752) that Franklin's experiment had succeeded exactly as predicted and that it had now been established without question that Franklin was right, that lightning is an electrical discharge.

The King of France ordered that a special note of thanks be sent to Franklin. Needless to say, the stunning success of this demonstration added to Franklin's reputation as a scientist and was a factor in winning additional adherence to his theory of electrical action at the expense of the then-reigning theory of Nollet's. Certainly, Nollet could be considered as a scientific opponent of Franklin's and perhaps even as an enemy, a term used by Franklin in later describing these events.

Almost all accounts of this extended episode are written from the point of view of Franklin's presentation in his "Autobiography." He wrote that Nollet "could not at first believe that such a work came from America, and said it must have been fabricated by his enemies at Paris, to decry his system." Note here Franklin's use of the word "enemies"! In context, however, it seems clear that Franklin was referring to Nollet's rivals at the Court, rivals for patronage or royal favor, but not enemies solely—or even primarily—with respect to scientific ideas or scientific theories.

Nollet responded to the attack in the form of a book containing a series of letters addressed to Benjamin Franklin but—so far as is known—never sent to him. Here Nollet quite rightly complained of the omission of his name from the preliminary history. Assuming from the historical introduction to Franklin's book that his name was not known to Franklin, he introduced himself and set forth some of his major discoveries and principles. He described in some detail the principal experiments he had made and he set forth for Franklin (and for his readers) the main points of his own theory. This theory differed basically from Franklin's because it envisaged a kind of electrical "aether" which was put into what he called conditions of "efflux" and "afflux." In particular, Nollet described experiments which he believed exhibited results which were contrary to those reported by Franklin and which, in his opinion, showed the inadequacy of Franklin's concepts and the theory of electrical action based upon them.

It is an irony of history that Nollet's research in electricity had actually elicited praise from Franklin in a portion of one of his communications which had not been included by Collinson in the printed book. Thus Nollet never knew of Franklin's original high regard for him.

Franklin himself never replied directly to Nollet. In one of the later supplements to his book on electricity, he published a response by a young protégé, David Colden, the son of Franklin's friend and fellow-scientist, Cadwallader Colden, Lieutenant-Governor of New York. Nollet continued the controversy by designing additional experiments which he believed refuted Franklin's ideas. These objections were published in a second volume of letters, which continued the attack on Franklin's experiments and his theory. Here we may have an example of the nearest case of a "scientist enemy" in Franklin's career.

At this point let me take note that a number of Franklin's discoveries aroused disputes about priority. For example, Jacques de Romas, a country official or civil servant who had the title of "assesseur au présidial de Nerac," claimed to have conceived of the kite experiment to test the electrification of clouds independently of Franklin, but after he had read of the published accounts of the success of the sentry-box experiment. Claims have been put forth that Father Procopius Davis, a Czech scientist, may have been an independent inventor of a form of the lightning rod. Even one of Franklin's major sponsors in England, William Watson, the leading British scientist in the field of electricity, alleged in a review of Franklin's book that many of the fundamental ideas were like his own.

A possible example of scientific enmity arose in relation to the shape of lighting rods. The introduction of lightning rods was beset with controversy from the very start. In part, this arose from a misunderstanding of the two functions of these rods. Franklin's original thinking about these rods was as follows. Assuming that clouds are electrified, Franklin concluded that a pointed and grounded metal rod would silently "draw off" charge from a passing cloud. This process would, so to speak, "disarm" the cloud, would remove its electric charge in the same manner that a grounded and pointed conductor would remove the charge from an electrified object in Franklin's laboratory. The only difference would be one of scale.

So convinced was Franklin of the outcome of the sentry-box experiment, that he published an account of this "disarming" function of rods in his almanac, "Poor Richard," even before an experimental test had been made. He himself then devised the experiment of the lightning kite, embodying the same principles as the sentry-box experiment, and performed it before he had heard of the success of the French experiments.

Once lightning rods had been installed, it was quickly discovered that they could perform another and even more useful function. If there should be a stroke of lightning, an elevated rod of iron, pointed and grounded, would attract the stroke and safely conduct it into the ground. Today this is recognized to be the primary function of Franklin rods or lightning rods.

One of the leading opponents of the use of lightning rods was the Abbé Nollet. No doubt one of his major reasons for opposing the rods was that they had been invented by Franklin. But Nollet, in addition, was guilty of a commonly shared confusion about the dual purpose of the rods. "By experiment after experiment," he wrote, "we have succeeded in touching the fire of heaven; but if through ignorance or temerity our profane hands abuse it, we certainly have cause to repent." He exclaimed, "What would we do if some grievous accident would cause us remorse; if consumed by unnecessary regrets, we would bring into being the Prometheus of the fable and his vulture!"

Nollet was aware that Franklin rods could attract the "electrical fire" from discharges by electrified clouds. Later experience had shown that in this way one could collect celestial electricity for experimental purposes. Since a Franklin rod thus "attracts" the lightning, Nollet and others argued, these rods are dangerous in that their "attraction" of lightning might cause damage to buildings that would otherwise not suffer from destruction by lightning. In short, Nollet expressed concern that a lightning rod might invite danger by attracting large surges of electricity.

In addition to such scientific objections, there were other objections which arose through superstitious fear or religious belief. There was then a widespread belief that the most efficacious means of averting the dangers of lightning was the ringing of church bells. Franklin, not surprisingly, seemed to take special joy in presenting the macabre statistics of the high mortality among bell-ringers. The Reverend Thomas Prince attacked the introduction of lightning rods in Boston. If, by means of lightning rods, you tried to avert God's wrath, he preached in a popular sermon, the lightning would accumulate in the bowels of the earth and produce an earthquake. "There is no escape from the hands of an angry God!" Procopius Davis had to remove his lightning rods because the peasants were afraid that the rods would produce a drought. Not all churchmen were opposed to the new invention, however, as may be seen when the basilica near Assisi was struck by lightning and had to be rebuilt. Pope Pius VI provided funds for the restoration but only on condition that "electrical

Franklin rods" be installed to prevent any future destruction by lightning. A marble tablet sets forth in Latin the condition imposed by the Pope, that he donated the funds only if protective lightning rods were to be installed.

The controversy over lightning rods that most nearly exhibits enmity occurred in England in the years just prior to and during the American Revolution. At issue was not the efficacy of lightning rods as such but the scientific question of whether the rods should end in points or knobs. In 1772, the Royal Society was asked to recommend means of protecting the powder magazine at Purfleet from lightning. There was just cause for alarm since a similar magazine at Brescia had recently been destroyed by the effects of a lightning stroke. An official committee (of which Franklin was a member) recommended the use of pointed conductors with metallic connections leading into the moist earth below the surface.

One member of the committee, Benjamin Wilson (an artist as well as an electrician, who painted a very charming portrait of Franklin) filed an official objection, but only to that part of the report recommending points. Wilson reverted to the old argument that "Dr. Franklin," in his experiments on lightning, had used a pointed rod in order to *"invite,* or *bring down and collect the lightning,* so as to make experiments upon it." For protection, however, Wilson considered it unsafe to use points as the ends of lightning rods because of "there [their?] great readiness to *collect the lightning in too powerful a manner.*"

The committee and other electrical specialists rebutted Wilson's arguments but in the end the controversy became political. A person's opinion as to "blunts" or "points" became an index of his political opinion with regard to the American cause. George III entered the fray ordering blunt conductors fixed on the royal palace. The King is said to have demanded that the president of the Royal Society, Sir John Pringle, use his influence to reverse the decision of the Royal Society. Pringle is said to have replied that His Majesty could change the laws of the land but could not reverse or alter the laws of nature. So intense was the dispute that Pringle was forced to resign his presidency.

Franklin himself did not enter the dispute. In a letter written from Passy in 1777, while the controversy was still raging in England, he declared "I have never entered into any controversy in defense of my philosophical opinions." Believing that his opinions should "take their chance in the world," he declared: "If they are *right*, truth and experience will support them; if *wrong* they ought to be refuted and rejected." Then, shifting from his role as scientist to politician, the American envoy to France continued: whether "the King's changes his *pointed* conductors for *pointed* conductors for *blunt* ones is . . . a matter of small importance." In fact, if Franklin "had a wish about it, it would be that he had rejected them altogether as ineffective." It was only "since he thought himself and family safe from the thunder of Heaven," Franklin concluded, that the King "dared to use his own thunder in destroying his innocent subjects."

No doubt, however, the wits carried the day. I conclude with a little epigram then circulating in England:

> While you, great George, for safety hunt,
> And sharp conductors change for blunt,
> The nation's out of joint:
> Franklin a wiser course pursues,
> And all your thunder fearless views,
> By keeping to the *point.*

Bibliographic Note: On Franklin's scientific career and the significance of the lightning experiments, see my *Benjamin Franklin's Science* (Cambridge: Harvard University Press, 1990); I have given a detailed presentation of Franklin's research in electricity in my *Franklin and Newton—an Enquiry into Speculative Newtonian Experimental Science and Franklin's Work in Electricity as an Example Thereof* (Philadelphia: The American Philosophical Society, 1956; Cambridge: Harvard University Press, 1966). An evaluation of Franklin's contributions to electrical science against the background of the times is given in John L. Heilbron: *Electricity in the Seventeenth and Eighteenth Centuries—A Study of Early Modern Physics* (Berkeley/Los Angeles: The University of California Press, 1979). On the Newton-Leibniz controversy, see A. Rupert Hall: *Philosophers at War—The Quarrel between Newton and Leibniz* (Cambridge, London, New York: Cambridge University Press, 1980).